Maternal Mercury Transfer in a Native Viviparous Fish: ecological risk and interactions with non−native species

Collin A Eagles−Smith

Public Comments

No public comments were received for this proposal.

Collaboration Panel Review

Proposal Title

#0074: Maternal Mercury Transfer in a Native Viviparous Fish: ecological risk and interactions with non−native species

Final Panel Rating
inadequate

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

This is a $1.4 million multi−investigator proposal includes USFWS and UCD that addresses whether maternal mercury transfer in tule perch has population level impacts. One set of tasks is lab tasks and the other field studies. All of the participants are critical to the overall effort and there is inter−dependence among tasks.

Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

The proposal presents an overall conceptual model as to why each of the projects is important to understand tule perch populations. The boundaries of the study plans are clearly defined, and timing doesn’t seem critical. What is lacking is

a good description of how the data from all the studies will be integrated together at the end.

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

Project management is not sufficiently discussed and nor budgeted. A total of $1000 of labor is budgeted for a $1.4 million project. The lead on the proposal is paid through a sub−contract from UCD, and his role is somewhat vague. The leads at UCD are very high−powered individuals who are totally unfunded by this project.

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

There was no CV issued for the lead PI. There was some indication that he would receive his PhD this summer. While the lead UCD players are among the best in the field, their time on this project is totally donated. Presumably they are also doing other projects. How much time will they have to oversee this project? How will Eagles−Smith be able to make decisions as their sub−contractor, having just received his PhD? While the UCD leads are excellent, the majority of the people doing the work that CalFed will pay for are undocumented (Student VI). It’s going to be hard to get this project up and running in a very timely manner, when most of the leads have yet to be hired.

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

The normal mix of papers and talks is proposed with the addition of the IEP newsletter. There's no indication of an integrative publication, simply some papers.

Additional Comments:

Collaboration Panel (Discussion) Review

Both reviewers rated the proposal as inadequate. Both felt unsure about the potential success of a project that consisted of a large, high-powered team managed by a subcontractor; it was unclear how the information was to be integrated together at the end of the study; project management was not discussed or budget explanation was inadequate; participation of organizations seemed to be arbitrarily assembled. Overall, the project structure and collaboration is unwieldly.

Secondary reviewer did give credit for different departments within a single organization collaborating together. But as review panel debated the nature of true intent of collaboration as described in the PSP, it concluded that this proposal plan as written was not truly collaborative.

Technical Synthesis Panel Review

Proposal Title

#0074: Maternal Mercury Transfer in a Native Viviparous Fish: ecological risk and interactions with non−native species

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Tule perch, a native species, were once numerous and widely distributed in the San Francisco Bay/Delta system. The PIs suspect that methyl Hg, shown to bioaccumulate in several species in system, may be one of the causes of the decline. They present a timely study designed to assess the effects of MeHg transferred maternally on the ability of tule perch to reproduce, effectively forage and avoid predation. Sub−lethal effects of MeHg on fish and wildlife are an emerging area of study and this team has ample experience and a proven track record to take on the project. The laboratory studies, the strongest portion of this complex proposal, attempt to establish biomarkers of MeHg exposure. The wide distribution of tule perch makes them an good indicator specie in the Bay/Delta and the authors plan to ground−truth their laboratory studies by trying to better understand the distributions in the field in relation to Mehg contaminantion and possible effects of invasive centrachids. Transferability of this approach to the field would make a powerful tool for assessing stressor impact and management concerns. While this proposal is well−justified and a better understanding the sublethal effects of maternal transfer in this viviparous fish is warranted, reviewers had significant concerns about the field application portions of the study. One reviewer suggests

that the field sampling portion of the study was merely a correlative study and will not truly be able to determine the causes of tule perch decline in the Bay−Delta. The lack of understanding of small scale population dynamics in the field is a large concern, especially when dealing with the combined effects of another specie (native or non−native) in the field. A separate pilot study could possibly be designed to look at tule perch abundance. Reviewers made several suggestions for strengthening the proposal including applying biomarkers from lab studies directly to field populations to assess stressors in tule perch.

Additional Comments:

The panel felt that the laboratory portion of this study could have stood alone and achieved a status of Above average. The field applications and invasive species sections clearly detrated from the well−designed lab studies.

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Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Maternal Mercury Transfer in Native Viviparous Fish: Ecological Risk and Interactions with Nonnative Species

The panel liked that the proposal focused on a new area of mercury research: maternal transfer. Quantification of maternal transfer was considered an important goal. The panel also liked the focus on sublethal effects of mercury.

The reviewers felt that the researchers built their case well: they took data from earlier studies and data from their own preliminary study and built a case for the importance of their approach.

The panel felt that the survey of abundance of tule perch and mercury levels was not well designed or thought out.

The panel felt that the hypothesis of methyl mercury interaction with introduced species was not well justified. They did not expect that this relationship would be a very promising area of research.

The panel considered the field component the weakest part of the study. The proposal would have been rated higher (above

average) if the laboratory study alone would have been proposed. The field component is approximately half of the entire budget and is not well-justified.

Rating: adequate

Technical Review #1

proposal title: Maternal Mercury Transfer in a Native Viviparous Fish: ecological risk and interactions with non−native species

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**This proposal has five broad objectives that are all scientifically interesting, clearly outlined and justified. These objectives range from laboratory studies focused on understanding the physiological and behavioral effects of maternally derived and dietary MeHG on viviparous fish to field ecological assessments of the spatio/temporal distribution of Hg concentrations. The breadth of the proposed research is certainly one of the major assets of this proposal. The field and laboratory studies provide a strong compliment to one another providing the groundwork for a broad impact of the eventual data sets.** **The hypotheses that are generated from the five main objectives are clear and appropriate to the questions being answered. However, there are three concerns that the hypotheses do raise. The first, concerns objective 1, Hypothesis 1, where the authors write that "...maternally derived Hg cause physiological stress response in young fish." This statement implies that the authors are examining chronic stress as the mechanism though which low levels of MeHg are causing impairment. Furthermore, this statement indicates that the authors intend to study the activation of the hypothalamic−pituitary−adrenal axis and measure either catecholamines or glucocorticoids as a measure of stress response activation. Since these measures are not included in the proposal, rather physiological**

	endpoints (such as impairment in growth, reproduction, etc.) are measured the terminology is misleading and not appropriate. The second question concerns Objective three. There was no a prior hypothesis (es) concerning the effect of MeHg on the behavioral or growth parameters that are measured. These studies are a significant part of the project and need clear hypotheses. The third question concerns Objective four, hypotheses three. The authors hypothesize that the combined effects of Hg and Predators will have a synergistic effect. This hypothesis does not have any justification within the proposal of why a synergistic rather than additive effect is assumed.
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**This study has a sound basis within the existing scientific literature concerning the effects of Hg contamination on fish. The authors provide an excellent conceptual model justifying the scope and breadth of the study. Given that this proposal is grounded in a large knowledge base on Hg contamination and toxicity it is reasonable to implement this as a full−scale project. Mercury is a wide spread contaminant within the San Francisco Estuary making it a significant factor in relation to the over−all health of the Tule perch population. Thus, this proposal is clearly relevant to the goals of CALFED.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the

approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**In general, this proposal has a clear, well-designed set of experiments to test the set of five objectives and their accompanying hypotheses. There is a logical flow to the experimental layout with initial laboratory studies providing the foundation for further study. The combination of field and laboratory projects are designed to compliment one-another with data feeding from one set of studies the next. The description of the field-projects are somewhat vague, but the experience of the P.I.s running this proposal indicates that they will be able to accomplish the tasks set forth in the proposal.** **There are a few minor concerns about the experimental design of the laboratory studies. The authors do not make it clear whether the Tule perch that are used in the Task 2 laboratory studies will have any prior exposure to mercury contamination and if so, what methods will be utilized to control for this additional mercury load in the experimental fish. In Subtask 2B, it is not stated whether the 10 fish randomly sampled for the monthly sampling will be males, females are a combination of both. There is a strong likelihood that males and females will carry different body loads of MeHg. The females, particularly after spawning may be able to reduce their body loads of MeHg compared to males if there is significant maternal transfer of the contaminant. Furthermore, males and females may respond differently to MeHg in terms of their endocrine response making it difficult to lump the sexes together in terms of measuring**

	reproductive dysfunction. Thus, the authors should strongly consider looking at the males and females separately. A final question concerning subtask 2B is why the authors are not measuring glucocorticoids in the plasma. Throughout the proposal the authors are referring to the stresses that the fish encounter after exposure to MeHg making it a logical endocrine endpoint to measure. **This proposal, despite the few concerns, will likely provide novel information about mercury toxicity. Especially concerning the effects of low-levels of mercury contamination on a potentially vulnerable species in the Tule perch. The wide array of physiological parameters that the researchers are proposing to measure will provide a wealth of new information. In addition, the combination of field and laboratory studies raise the impact and utility of this study making the eventual results that much more interpretable and applicable to regulatory agencies.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**This proposal is ambitious with a wide breadth in techniques and scope making it a technically difficult task to complete. However, the team of P.I.s, researchers and staff that has been assembled indicates that the likelihood for success is very high. Based upon their past research success and their knowledge base this project is well within the grasp of the authors.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**While the authors do not provide information about how the data will be analyzed for most of the studies, it is clear from past work that the authors are quite competent at data analysis. The one potential stumbling block is the design of sub−task 2. The initial experiment is designed to utilize four tanks with each tank containing one of the four treatments. This raises a potential confound in the possibility of tank effects that cannot be statistically disassociated from treatment effects. If feasible, it would be beneficial for the researchers to include additional duplicate treatment tanks. The experiments (with only a few exceptions mentioned earlier) are well designed, maximizing the ability of the researchers to gather scientifically sound datasets.**
Rating	**excellent**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The authors consider the broader impact of their eventual data set and indicate that it will be widely disseminated to the California Bay Delta Authority and to the public. The fact that the researchers are working at both UC Davis and the US Fish and Wildlife indicate that the data will be utilized, not just in the academic world, but will also contribute directly to the management agencies.**
Rating	**excellent**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The track records for Drs. Moyle and Cech are outstanding and demonstrate that they have the experience and the expertise to accomplish the many tasks of this project. The additional P.I.s, while not as established, do have appropriate backgrounds and documented expertise to accomplish the portions of the proposal under their control. The facilities and infrastucture at UC Davis and the US Fish and Wildlife Service are certainly sufficient to carry out the proposed research.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is large, but appropriate for the amount of work proposed and the numbers of personnel needed for a project of this scope**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**This proposal is excellent. There are a few minor issues with the experimental design, however they are not so egregious that they would jeopardize the**

	outcome of this project. The track record of the principal investigators is outstanding and justifies the confidence in their abilities to complete the tasks within this proposal. The combination of laboratory and field studies makes this an attractive proposal since the two research fronts provide insight that either one alone would not. The goal of the study is to understand the interactions of sublethal effects of methyl−mercury and non−native species on the physiology and behavior of an important native species the Tule perch. This goal has scientific merit as well as relevance to the enhancement of fish populations in general.
Rating	**excellent**

Technical Review #2

proposal title: Maternal Mercury Transfer in a Native Viviparous Fish: ecological risk and interactions with non−native species

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Yes; Five coherent and testable hypotheses (= objectives) are presented that predict direct effects of Hg and form the basis of the proposed work. The heart of the work plan entails combining field and laboratory analyses involving exposure to controlled doses of dietary MeHg, quantifying effects of non−native species on exposed and control fish, in conjunction with assessing Hg burdens in resident populations that coexist with introduced species, i.e. looking at the interaction between Hg exposure and impacts of introduced species.** **The timeliness of such a study is increased with the recent attention focused on Hg contamination in a news report in Science magazine on Jan 26, 2005.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Is the study justified relative to existing knowledge?** **Yes; literature survey appears up to date.** **Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work?** **Yes: (The fish in the conceptual model look more like acanthurids than centrarchids; the opacity "device" is clever).** **Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?** **The preliminary work on (unpublished data of Eagles-Smith et al) on maternal body burden in embryonic tule perch (Fig. 1) strengthens justification for the proposed study.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach in this proposal is exceptionally strong and well thought-out. The experimental (including the physiological and behavioral measures) and field observation components of the work appear reasonable in terms of scope, plan, detail, and ambition, using standardized methods. The only weak/incomplete component is the proposed test of competitive effects with bluegill, which lacked detail. Given the infrequency with which demonstrable effects of competition have been shown in fishes, this component**

	holds minimal promise. Similarly, the field component on potential dietary overlap with competitors is equivocal without information on resource availability (also, the likelihood that juvenile elasmobranches are competing with bluegill is low).
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	I can see no reason why the investigators would not be able to complete the proposed tasks given the personnel and time frame presented.
Rating	excellent

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	Given the publication records and activities of three of the four main participants, there is little reason to doubt that the information arising from this work will be disseminated scientifically, within the CALFED community, and to the public.

Rating	**excellent**

Additional Comments

Comments	**This proposal focuses on maternal transfer of Hg to offspring in a viviparous, native fish, the tule perch, and possible impacts on reproduction, feeding, and predator avoidance in Hg-exposed juveniles, particularly as they relate to interspecific interactions. Collateral information will be obtained on various life history and population attributes of the study species. A key assumption is that Hg level standards have focused on effects on adults, whereas juveniles may be more sensitive, as may reproduction, which combined would have substantial direct and indirect impacts on population dynamics. Comparative data will be collected on other live-bearing fishes such as other embiotiocids and sharks and rays.** **The proposal purports to address Core component 5 of the CALFED program, which focuses on ecological risks associated with the adverse ecological impacts of high priority stressors such as methylmercury, motivated in part because of concern over mobilization and transformation of Hg that occurs during restoration projects. The investigators point out that Hg levels in the estuary are relatively low but that early life history stages of fishes may be inordinately susceptible to contaminants and these effects may have been underestimated and underappreciated and in fact largely neglected. The investigators also emphasize that current knowledge of maternal transfer to young is based on information from egg bearing fishes, whereas they present preliminary results indicating that "young viviparous fishes are exposed to significantly more (4x) MeHg . . . than oviparous fishes because these young receive nutrients directly from the mother's bloodstream during their gestation." The investigators also maintain that standard response metrics to contaminants have been insufficiently**

	sensitive to show sublethal impacts of exposure and that behavioral, ecological, and physiological stress measures would be more sensitive at the concentrations found in the field. **An unstated but inferred factor that enhances the value of the proposed work is the inevitable analogy that will be drawn between live-bearing fishes and mammals (particularly humans).**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The expertise and publication records of Moyle and Cech are unassailable; Slotton's publication record w.r.t. Hg is impressive. For Eagles-Smith we only have the statements in the proposal about his qualifications, with little validation. Supporting personnel are extensive and well-trained. Coordination with agencies is another commendable aspect of the proposal.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**I do not feel qualified to give a rating. I've looked it over and see nothing glaring. My only hesitation is the salary for Eagles-Smith, given his relative track record compared with other personnel. But given the proven track records of the other p.i.'s and their apparent faith in Eagles-Smith, I am satisfied.**
Rating	**not applicable**

Overall

Provide a brief explanation of your summary rating.

Comments	**My only reservations being the competition component, which I see as holding minimal promise, and some questions about the post-doctoral person, whose c.v. was not included (or perhaps I just didn't find it). The importance of the post-doc is obviously crucial to the project given the responsibilities and interests of the other senior personnel.** **CALFED's focus on Hg contamination is obviously justified, and the literature on non-lethal impacts on fishes is decidedly thin. This very thorough, in both depth and scope, proposed study will constitute a substantial addition to that literature in addition to meeting CALFED objectives**
Rating	**excellent**

Technical Review #3

proposal title: Maternal Mercury Transfer in a Native Viviparous Fish: ecological risk and interactions with non−native species

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The objectives and hypotheses of the proposal are clearly stated and consistent. The relationship of the proposed research to the goals of the CAL−FED program are clearly articulated. Methylmercury contamination and introduced exotic species are potential stressors to native fish species and little is known about their effects, either individually or in combination, to native fishes of the San Francisco Estuary. The principal investigators propose a novel investigation combining both laboratory and field work to assess the interactive effects of predatory and competitive stress by exotic fish and methylmercury contamination to the tule perch, a native viviparous fish distributed widely in the Bay−Delta region.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**(1) Overall, the proposal justification is well−described, rational, and makes a strong case for pursuing this line of investigation. The proposal provides an extensive review of**

	the pertinent, recent literature that demonstrates that dietary methylmercury at realistic environmental concentrations can alter reproduction and physiology of fish. Less information (one paragraph, page 7), however, is presented to support the contention that embiotocid fish populations are declining throughout the Estuary or that exotic fishes can competitively exclude native fish or extirpate native fish populations through predation. (2) Because "much is unknown about the overall, abundance, distribution and movements of tule perch" (page 13) in the San Francisco Estuary, the field studies may be more appropriate to a pilot study to better assess the feasibility of this aspect of the project.
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The primary goal of this project is "to quantify the relative impacts and interactions of both mercury and non-native fishes on a potentially sensitive native species" (page 2). The individual laboratory experiments are generally well-designed and appropriate for examining the effects of dietary methlymercury on the autecology and physiology of tule perch, although there is no indication that many of the endpoints will be compared to growth, reproduction, or survival of the fish. However, a major weakness of the proposal relative to the stated goal is that the laboratory investigations (1) do not assess the effects of centrarchid competitors or predators on tule perch, and (2) do not establish nor**

quantify the interactive effects of methylmercury and exotic fishes on the native fish species. Moreover, the field study is primarily a correlative investigation between tule perch abundance, methylmercury, and centrarchid abundance—causal linkages between laboratory and field investigations are not established (e.g., potential alterations in biomarkers measured in the lab are not measured in the field). As stated in the proposal (page 7), "Several other explanations have been postulated to explain this decline [of tule perch], including habitat loss, climate change, and contaminants." Unfortunately, I do not believe that the approach presented in the proposal will identify the potential causal agent(s) for the decline in tule perch abundance. The approach could be strengthened in several ways. (1) Establish a linkage between several of the biomarkers measured in the lab (e.g., T. E2, GSI, enzyme activities) with tule perch reproductive success, growth or survival and then measure these biomarkers in the field populations of fish. As the proposal correctly states (page 7), "A major concern of many laboratory−based ecotoxicology studies is their lack of applicability to environmental reality, while a common concern of field−based ecotoxicology studies is their lack of quantification and difficulty in establishing causal linkages with potential effects". In its current form, the proposed methodologies do not adequately address this issue. Establishing the relationship between the important endpoints of growth, reproduction or survival, with biomarkers that can be measured in the field will help build a convincing case that methylmercury does or does effect wild tule perch populations.

(2) Establishing a cause−effect relationship between centrarchids and the decline of tule perch is more problematic. Would it be possible to conduct mesocosm, enclosure, or pond experiments similar to those that Earl Werner, Gary Mittelbach, and others did in the 1980s assessing competitive interactions and resource

partitioning by different species of centrarchids? Results of a replicated experiment would better establish a cause–effect relationship for any observations made from a correlational field study of perch and centrarchid abundance.

Minor comments: Subtask 2A: Quantifying maternal transfer (1) As the fish in this portion of the laboratory study are used for measurement of multiple endpoints in other subtasks, careful consideration of experimental design is warranted. Adult fish will be randomly assigned to four tanks and then fed one of four experimental diets. No mention is made of the type of statistical analysis (if any) to be used in analyzing the data, but the experiment is potentially pseudoreplicated. For the purpose of statistical analysis, the experimental unit is the tank, not the fish (The contaminated food is added to the tank and the fish within the tank can not randomly and independently of each other consume the different diets). Therefore, there is only one experimental unit per treatment and comparison of treatment effects (e.g., ANOVA) is not possible.

Subtask 2B: Effects of dietary MeHg on tule perch endocrinology, performance, and reproduction. (1) See comment for subtask 2A. (2) Would it be possible to combine measurement of dietary MeHg uptake rates by juveniles with subtask 2A. I.E., raise juveniles to sexual maturity and quantify maternal transfer? This experiment is properly replicated (3 tanks/treatment).

Subtask 3A: Mercury Assessment Using age–1 fish is a good idea and will reduce size–related variability in Hg concentrations. But will 10 fish provide the level of precision that is required to assess differences in Hg concentrations among populations? A preliminary assessment of Hg in tule perch and power analysis would provide some idea of sample size requirements.

Subtask 3A: Habitat Assessment "…need to evaluate or

	correct for habitat variability in habitat parameters…" This portion and not well defined. How do you correct for habitat variability? Are the habitat requirements of tule perch well defined? E.g., HSI models or other methods of relating tule abundance to habitat metrics?
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach for the laboratory investigations are fully documented and technically feasible with a high likelihood of success. The PIs have extensive experience and are well qualified to conduct these types of laboratory investigations. Because of the lack of information on tule perch populations, the success of the field studies to quantify population size and reproductive success relative to a methylmercury gradient is less certain, but still possible.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the

project?

Comments	**The project will provide information on (1) the sublethal effects of dietary methylmercury on tule perch and (2) fecundity and abundance estimates of wild populations of tule perch along a mercury contamination gradient. This information is important in identifying potential stressors to native populations within the Estuary.**
Rating	**good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**PIs Moyle and Cech have a long and distinguished record of research and publication in fish ecology and physiology. PI Slotton is an established scientist with a solid record of project management and investigation of heavy metal bioaccumulation and cycling. No CV for Co-PI Eagles-Smith was provided. The investigators have assembled a multi-disciplinary group for this investigation. They have the facilities, experience, and track record necessary to accomplish the project.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The overall budget is reasonable and appropriate for the proposed work. However,**

	without knowing more detail, the administrative costs for the subcontractor (15 days x 3 years = $33480) seems excessive.
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**The purpose and intent of this project is excellent. The PIs have excellent credentials and track record and are fully capable of achieving the proposed laboratory. However, several issues compell me to rate this proposal as good to very good rather than excellent: (1) The approach does not adequately address the competitive and predatory effects of centrarchids on tule perch; (2) there is little to link the laboratory and field results; (3) the "adaptive approach" (page 15) required in the field indicates that a pilot project and preliminary data collection would be prudent.**
Rating	**good**